

May 1, 1953

Dear Luca:

I was very sorry to hear of the disruption in your family and your work.

As to the paper, I shall be ~~disapase~~ disappointed if its appearance in the proceedings of the congress is prevented, but only if as a consequence of an excessive regard for my privilege as co-author. Your letter has just this minute arrived, and I have not had an opportunity to study the draft. However, I would have every confidence in the rather amazing coincidence of our attitudes, and would not require an inspection of the final draft. It may be questioned whether the Proceedings are the best place for publication, as they are usually delayed, but Chain probably has in mind a special symposium on chemotherapy, and this would be an excellent place. I will look at the draft during the next couple of days. However, I am extremely fatigued and my literary cortex is not-up-to par. After completing my previous notes on this, I have had to take part in two successive revisions of a ms. with Stocker, and to complete a paper with Edwards. This makes me now rather unenthusiastic about still another paper for the Rome Congress. However, may I suggest the following: 1) Invite Stocker (London School of Hygiene) to give a 40<sup>0</sup> paper on Salmonella transduction [while with which he is quite familiar, both the background, and details of his own work,] and 2) if you wish, we can jointly re-present the substance of our Genetics or JGM paper as a discussion of the E. coli story. Come to think of it, Stocker has also had a year's experience with pneumococcus transformation in MacLeod's laboratory, with some very interesting, so far unpublished data. The most balanced approach might be to ask Stocker to present the comparative genetics of Salmonella, pneumococcus [and, by review, Hemophilus], asking him to emphasize his own experimental findings. I have no opposition to Alexander as well, except that this may tend to over-balance the program, and she has been rather naïve in genetic interpretation. If we are to give 2), I must ask you again to adopt the chief burden of preparing it. However, the substance is already in 2 papers, and there is no need to publish at any great length. All of this is predicated on our own disappointing conclusion that we must ~~abed~~ abandon hope of travelling to Europe this summer. We simply have not been able to find the funds. The economy drive of our new administration played some part in preventing the possibility of official airforce transport, and the Nat. Science Foundation offered a grant of \$300, less than ~~25~~ 20% of our minimum estimated expenses. [Frankly I am a little relieved. The thought of now making all the necessary preparations would have been terrifying.]

You are probably wiser not to join in polemic with Yudkin. Spiegelman tells me that Hinshelwood himself (at the SGM) severely criticized Yudkin's approach as an untenable compromise. I have only Sp.'s version of the story, and have not yet seen the paper. I am sending the filter promptly. Partly as a result of my own accident, I will have it prepared by our glassblower to shorten the tube, and leave it ready for the simple sealing on of the vertical arms.

I will try to ignore the rhetoric of your draft. I hope Jinks will not seriously influence any other aspect than this. E.G. in his paper with Rees he omits the most likely explanation of heterokaryotic stability: that the hyphae with the most adaptive ratios (achieved by sampling fluctuations) grow most rapidly as a whole. His criticism of reverse-mutation work was specious. But he has also made some affirmative contributions.

To return to the subject of the program. Szilard is no longer seriously concerned with mutagenesis; Novick has made the most important contributions to spontaneous mutation study, is an excellent speaker, and will be in Rome. I would urge that you invite him. You may have to ask Demerec, as ~~an~~ a matter of polity: assez dit. Pontecorvo is much more likely to be at Rome than Roper. I am not certain that lysogenicity has to be on the genetics program (it will probably be considered elsewhere, and you should verify this before going any

FOLD SIDES OVER AND THEN FOLD BOTTOM UP AND SEAL.  
NO OTHER ENVELOPE SHOULD BE USED.

~~any~~ further. But if lysogenicity is taken up, you will have to ask Lwoff. Instead, it might be more appropriate to have the topic genetics of phage, and to consider Visconti, Levinthal, Doermann as among the younger men. Niscolo would do very well, I think.

I do not want to de-emphasize this by leaving it to the end, but perhaps this is not the time to ask you to think about it. From the experience of Spicer and ES Anderson, you would likely have an excellent likelihood of support from the UN World Health Organization for a travelling fellowship, even for three to six months. Your position at a public health laboratory greatly reinforces these chances. I would urge you to look into the matter, if there is any occasion when you feel you have the time. The program seems to be administered from Geneva: address the Director-General, W.H.O., Palais des Nations. I would gladly try the same, but the fellowships are preferably given to countries other than the US and UK.

You may expect my comments with a few days.

Amicablement,

*Joshua*